Using propensity matching estimators to evaluate the impact of privatisation on wages

Monteiro, Natália Pimenta

Using propensity matching estimators to evaluate the impact of privatisation on wages

Journal:	*Applied Economics*
Manuscript ID:	APE-06-0587.R1
Journal Selection:	Applied Economics
Date Submitted by the Author:	17-May-2007
Complete List of Authors:	Monteiro, Natalia; University of Minho, Economics Department
JEL Code:	J31 - Wage Level, Structure; Differentials by Skill, Occupation, etc. < J3 - Wages, Compensation, and Labor Costs < J - Labor and Demographic Economics, J45 - Public Sector Labor Markets < J4 - Particular Labor Markets < J - Labor and Demographic Economics, L33 - Public versus Private Enterprises; Privatization < L3 - Nonprofit Organizations and Public Enterprise < L - Industrial Organization
Keywords:	privatisation, wages, portuguese banking industry, propensity matching estimators

Editorial Office, Dept of Economics, Warwick University, Coventry CV4 7AL, UK

Using propensity matching estimators to evaluate the impact of privatisation on wages*

Natália Pimenta Monteiro†

Abstract

Whether the transfer of ownership rights to the private sector leads to a decline or increase in wage growth is theoretically ambiguous, given that the outcome depends on the uncertain interaction between firms and workers. Using propensity matching techniques, this paper investigates the effects of privatisation on wages in the Portuguese banking industry. The empirical results, obtained from *Quadros de Pessoal* for the period between 1989 and 1997, generally show a negative (positive) short-run (long-run) effect of privatisation on relative wage growth for both men and women retained in the privatised firms. Moreover, the results show that the most educated and experienced (oldest) workers, as well as those in the high skill occupational categories, were more likely to experience a negative wage effect.

Keywords: Privatisation, Wages, Portuguese banking industry, Propensity matching estimators.

Jel classification: J31, J45, L33

*I thank Mark Stewart, Odd Rune Straume, Ian Walker and participants at several seminars and conferences for valuable comments. I am indebted to the Ministério do Trabalho e da Solidariedade for allowing the availability of data from *Quadros de Pessoal*. Financial support was provided by the Ministério da Ciência e Tecnologia under the grant BD/SFRH/2000/1291.

†Address: Department of Economics, University of Minho and NIPE, Campus de Gualtar, 4710–057 Braga, Portugal, telephone: +351 253 614 539, fax: +351 253 601 380, e-mail: N.Monteiro@eeg.uminho.pt.

1 Introduction

Despite the large and prolific literature on privatisation, the analysis of the causal effect of privatisation on wages remains fairly neglected.[1] This is somewhat surprising, since the transfer of ownership rights to the private sector has been perhaps the most important structural reform, introduced worldwide, in the increasing use of markets to allocate resources.[2] More importantly, its implementation has frequently been met with fierce resistance from both labour unions and local communities, and has attracted intensive press attention. Whilst policy-makers tend to advocate gains in terms of firms' internal efficiency and profitability, labour unions fear adverse workforce adjustments, including either displacement of jobs or reductions in pensions or wages, as a result of the restructuring process. Perhaps the relative lack of empirical research on this controversial topic merely reflects the unavailability of appropriate data. Typical research on privatisation uses data from firms' annual accountancy reports, which, at best, contain crude labour force information.

At the theoretical level, the relationship between privatisation and labour market outcomes is not obvious; privatisation does not necessarily cut jobs or lower wages. Employment and wages may decline as privatisation implies a shift in the public firms' objective function towards profit maximisation, which affects the outcome of wage bargaining (Haskel and Szymansky 1992, 1993). However, if workers are willing to put in more effort after privatisation, then firms may settle for higher wages (see, e.g., Goerke, 1998, De Fraja, 1993, and Haskel and Sanchis, 1995). Similarly, if new ownership brings fresh capital and expertise, such changes are likely to generate growth and job creation.

The present paper contributes to this discussion by implementing a variety of increasingly popular non-experimental methods, labelled propensity matching estimators, to assess the impact of privatisation on wages. In particular, this study re-visits the effects of privatisation in the Portuguese banking industry, where the already accomplished reform is considered a "valuable experience for other countries", since "the main reform objectives were met" without "the concomitant financial instability experienced by many OECD countries"(OECD, 1999, page 64). In this way, this study also contributes to the long-standing debate in the literature, until now almost exclusively confined to the evaluation of active labour market policies, over whether treatment effects in observational studies can be reliably evaluated

[1] Some notable exceptions include Brainerd (2002), Haskel and Szymansky (1993), Ho *et al.* (2002), La Porta and Silanes (1999), Monteiro (2004), Parker and Martin (1996) and Peoples and Talley (2001). Megginson and Netter (2001) survey the empirical literature on privatisation.

[2] Megginson *et al.* (1994) provide an excellent historical overview of postwar privatisations in developed countries. For a study of privatisation effects in a large number of developing countries, see Al-Obaidan (2002).

without a randomised experiment.

The present study is empirically fruitful for several reasons. First, apart from the remarkable success of the above mentioned policy in the banking sector, the design of the privatisation program in this industry provides a promising opportunity for examining the effects of a change in ownership. Indeed, not only did privatisation *not* affect all public firms (there is still a large state-owned group), but it was also implemented continuously over eight years. Hence, this *partial* and *ongoing* privatisation design permits us to pair individuals both in the same labour market and with common public employment status. Therefore, we avoid the potential bias resulting from labour market mismatch (Heckman *et al.*, 1998) commonly observed in observational studies, and the self-selection bias inherent in the classical model of Heckman (1979) in the context of private and public sectors.

Second, the adoption of propensity matching estimators is also economically appealing for analysing the impacts of privatisation. In fact, as privatisation is likely to cause disproportionate changes in the composition of the workforce in privatised firms (compared with public firms), we would prefer a strategy that is robust to this unequal employment composition variation. By pairing each program participant, according to observable attributes, with members of a comparison (non-treated) group, matching leads – in principle – to ex-post re-establishment of the conditions of an experiment. Therefore, this effect is naturally controlled for. Besides, matching is a flexible approach that avoids definition of a specific form for either the outcome equation, decision process or the unobservable term.

Furthermore, this class of estimators is also appropriate to appraise the effects of the reform over both the short and long run. Indeed, the original cross-section pairwise matching estimators have been recently extended not only to new multiple matching schemes, but also to the case of repeated cross-sectional or longitudinal data. These new modified versions, which will be described below, are less restrictive in assumptions and can thus produce more accurate estimates.[3] The original matching assumptions are well suitable for short-run effects of treatment, whereas these new extensions are likely to become more plausible as we attempt to pick up more persistent medium-/long-term effects of privatisation.

Finally, this class of estimators has heavy data requirements since the quality of matching estimates mirrors the quality/quantity of the variables employed. This paper uses data from a large dataset, *Quadros de Pessoal*, collected by the Portuguese Ministry of Labour and Solidarity. This extensive matched employer-employee database provides detailed information about each unit, firm or individual, during the period before and after privatisation.

[3]For details, see the original papers of Heckman *et al.* (1997, 1998a) or the discussions in, e.g., Smith and Todd (2005) and Blundell and Dias (2000).

Hence, it allows us to draw samples of different nature (cross-section and longitudinal) and implement the entire class of matching estimators. Moreover, as all treated and control units respond to the same mandatory employer report, there is no bias resulting from differences in survey questionnaires (Heckman *et al.*, 1998).

The rest of this paper is structured as follows. Section 2 discusses briefly the main features of the privatisation process and the labour relations prevailing in the Portuguese banking sector. Section 3 presents an overview of the assumptions and variety of the *matching estimators*. The data implementation issues are addressed in Section 4. Section 5 outlines and discusses the empirical results. Section 6 closes the paper, summarising the main lessons of this study.

2 Privatisation and the Portuguese banking labour market

The privatisation program was introduced in the banking sector as a further step in the successful reform of the Portuguese financial system (OECD, 1999). This structural reform, starting in 1984, aimed to put an end to the heavily regulated and nationalised system imposed in the industry after the 25^{th} April 1974 revolution. Less than one decade afterwards, when most of the deregulation reforms were already accomplished, including the dismantlement of the interest rate controls and the openness of the financial intermediation to the private sector, the privatisation program was then implemented.

The first privatisation law adopted in 1988 (law 84/88 from 20^{th} July) allowed merely partial privatisation of public enterprises as the state still retained 51 per cent of the equity. For this first phase of privatisation, the government selected four profitable firms, which included one medium size bank. In April 1990, after a second Constitutional Amendment laid down in June 1989, the *lei Quadro das Privatizações*, (decree-law 11/90 from 5^{th}April) was passed, allowing full privatisation of enterprises nationalised after 1974. The privatisation program was assumed to be an important mechanism for (1) improving the deteriorated performance of public economic units, (2) modernising and increasing their competitiveness and (3) widening the participation of Portuguese citizens in the ownership of enterprises, particularly among workers and small shareholders.[4]

The firms being privatised were first transformed into corporations, with a prior evaluation being made by two independent entities. However, in contrast with some other economic sectors (e.g., electricity and telecommunications), the government opted for a policy of no interference in the public firms during the period before privatisation (Naumann, 1995, and

[4] Sousa and Cruz (1995) describe and discuss the economic and financial situation of public enterprises.

Sousa and Cruz, 1995), leaving the economic restructuring for future private owners. In terms of scheduled order of privatisation, apart from those firms which were selected on grounds of performance indicators for the partial privatisation phase (OECD, 1989), there was no set schedule for subsequent firms' privatisation (OECD, 1991). Instead, the timetable was strongly affected by the economic and political domestic cycles, and by the international context.

By mid-1997, ten out of twelve public banks became fully private: two banks were privatised in 1991, three in 1994, and each of the five remaining banks were privatised in 1989, 1990, 1992, 1993 and 1996, respectively.[5,6] The most common privatisation procedure used, was public offer, and, to a much lesser extent, direct sale or public tender. The broadening share-ownership goal clearly desired by the authorities was not achieved; instead, a managerial-dominant type of ownership emerged (although the employees had the right to subscribe to some part of the capital of the privatised firm at preferential rates). In most cases, ownership returned to former Portuguese groups, which owned them prior to the nationalisation wave in 1974.[7] Due to this private-public-private ownership path, privatisation in Portugal is termed re-privatisation.

As a result of the divestiture reform, significant improvements in terms of productivity and efficiency levels were registered in the Portuguese banking industry. For instance, the OECD 1999 survey, referring to the commercial banking industry, reports a continuous increase in the productivity level (balance sheet total per employee), which allowed not only a reduction in operating/staff costs (from 1.53 per cent of average assets in 1991 to 0.98 percent in 1997), but also a remarkable improvement in the profitability rate (return to equity) after 1995. This global rise in the efficiency level of the industry is also confirmed by Pinho (1999), who nevertheless attests to an increase that is particularly more pronounced among privatised institutions.[8]

The developments at the ownership level conditioned the type of industrial labour relations prevailing in the industry, which are unique to Portugal. Covering three different geographical areas, the oldest labour unions in the mainland represent *all* employees in the bargaining process, regardless the ownership of the bank. Indeed, trade unions and a group

[5]This total number (ten) of firms privatised in the banking industry does not coincide with the eleven privatised firms reported by the OECD 1999 survey, due to the absence of one bank in the data.

[6]According to the privatisation literature, the date of the first tranche sale of each firm is considered the date of effective privatisation.

[7]International investors could buy a limited share of the equity, ranging from two to forty percent of sales.

[8]A contrary conclusion is reached by Kraft *et al.* (2006), with data from the Croatian banking sector, showing that privatisations did not have an immediate effect on improved efficiency.

of banks (employers), public and private (domestic and foreign), meet each year to negotiate the vertical collective bargaining agreement. This collective agreement, the most detailed and extensive in Portugal, regulates the employability conditions, the remuneration and the duration of work. In particular, it delimits the starting wage level and the compulsory wage progressions for each of its 18 levels of the 4 groups defined to cover all the banking workforce.

Beyond this broad scope of the collective agreement, banking unions also enjoy the strongest attachment in the economy. Indeed, the unionisation density has expanded markedly between the periods 1974-78 and 1991-95, from 71% to 106% (Cerdeira, 1997).[9] Despite this notable reinforcement, unions did not act against privatisation. The resistance offered was very limited, not coordinated, and mostly being made through internal speeches which were rarely reported in the national press. An interesting indicator of the tranquility is the total absence of any strike action during the privatisation reform.[10] However, unions improved their relative negotiated wage growth during the privatisation period. Indeed, during the period 1989-97, unions in the banking industry (rest of the economy) obtained an average annual growth rate of negotiated wages of 7.7% (8.3%), while in the pre-reform period (1981-88) they obtained 16.2% (17.4%).[11] A priori, the coordinated bargaining system should bring uniform wage levels across firms within the banking sector, although the positive differential between the wage defined by the collective agreement and the actually paid wage has widened since the early nineties (Aperta *et al.*, 1994).

In terms of labour outcomes, the main economic restructuring adjustments are illustrated in Table 1.[12] For comparison purposes, the public category refers to the two permanent public banks, whereas the privatised category includes the ten firms being privatised. In contrast to public firms, whose level of employment remained fairly constant from 1991 onwards, the level of employment in privatised firms dropped steadily during the reform period. Each privatised firm lost on average 732 employees between 1989 and 1997 (implying a 23 per cent rate of overstaffing), which corresponds to a loss of 92 employees per firm/year during the same period. Nevertheless, this downsizing of employment is accompanied by a significant increase in the total working hours and in the share of permanent full time workers, more notable in privatised than in public firms.[13]

[9] The unionisation density includes unionised, both active and retired, employees.

[10] Source: MSST, Greves Anual. Informação Estatística (Síntese), (various issues).

[11] Source: Own computations based on MSST, Relatórios e Análises, Regulamentação do Trabalho (various issues) and on bargaining contract data supplied by Sindicato Bancário do Norte (1981-1997).

[12] Unit of currency = escudos (PTE). 1 Euro = 200.482 PTE

[13] In some cases, the corporate economic restructuring involves the adoption of less secure job (human resource) practices, including either temporary or partial employment, in order to achieve more *flexible* industrial relations. Cam (1999), for example, reports significant jumps in the number of temporary posts

Table 1: Employment, wages and individual attributes during the privatisation period

	1989	1991	1993	1995	1997
Average employment					
Public	7 323	6 771	6 812	6 793	6 856
Privatised	3 884	3 733	3 663	3 425	3 152
Total working hours per month					
Public	148.3	146.7	151.4	145.6	156.4
Privatised	148.3	145.4	154.9	146.2	158.2
Full time status %					
Public	89.1	91.5	95.2	98.0	98.5
Privatised	83.3	98.5	96.3	98.4	98.7
Real hourly wage*					
Public	1455 (686)	1702 (770)	1883 (985)	2007 (932)	1895 (790)
Privatised	1321 (524)	13992 (614)	1733 (1073)	1841 (967)	1855 (882)
Logarithm of real hourly wage*					
Public	7.22 (.328)	7.37 (.372)	7.46 (.360)	7.54 (.326)	7.49 (.328)
Privatised	7.12 (.354)	7.23 (.361)	7.36 (.411)	7.44 (.365)	7.45 (.368)
Age**					
Public	40.7	42.5	42.7	40.4	40.9
Privatised	40.7	41.9	42.8	43.3	43.7
Tenure**					
Public	13.9	15.7	16.0	14.2	14.7
Privatised	14.0	15.2	16.0	17.0	17.5
Schooling**					
Public	9.7	9.8	10.1	10.7	10.9
Privatised	9.2	9.2	9.3	10.7	10.6

Source: Own computations based on Quadros de Pessoal, MSST (1989-1997).
* Standard deviation in parentheses. ** Computed in years.

The trend in the banking workers' wages is also clear: both public and privatised firms' workers experienced a strong (real) wage rise, mainly reflecting the fast economic growth observed in the economy, after Portuguese membership of the European Community in 1986. For privatised firms' workers, however, the wage increase is clearly more pronounced than for public employees. At first glance, this suggests a positive privatisation impact on the wage level. Between 1989 and 1997, privatised employees enjoyed a wage increase of 40 percent whilst public employees enjoyed a wage gain of 30 percent.[14] This convergence in payment level is particularly notable as important dissimilarities in terms of human capital attainments, present already in 1989 (before privatisation), became more evident after privatisation took place. Employees in privatised firms, even after the reform, are the least educated, the oldest and the most experienced in the banking sector. On the other side, the rise in the wage dispersion in privatised firms, when measured by the standard deviation of hourly wage, may suggest heterogeneous privatisation wage impacts. Notice that this simple analysis, besides not accounting for changes in the workforce composition, ignores the time elapsed since the introduction of the reform in each firm, which possibly mitigates dynamic privatisation effects.

3 Econometric considerations

Assessing the impact of privatisation on wages of workers, whose firms' ownership were transferred from state to private hands, requires making an inference about the wages that would have been observed had the privatisation program not been introduced. As one can not observe the wage paid to each privatised firm's employee in case the reform had not taken place, the establishment of the casual effect becomes a problem of inference with missing data.

To be precise, let us state formally this causal effect. Denote by W_{i1} and W_{i0} the wage paid to an individual i (outcome or variable response) conditional on the presence and absence of *treatment* (privatisation), respectively. D_i is a participation variable that identifies whether employee i received "treatment", i.e., whether she was employed in a firm that was privatised ($D_i = 1$) or not ($D_i = 0$). Finally, X_i represents, for each individual i, a set of attribute variables, such as gender or age, that are unaffected by the treatment under study. The missing data problem arises because it is impossible to form the impact of the policy for any $i'th$ individual, $\triangle_i = W_{i1} - W_{i0}$, as the observed wage for an employee i is given by

in the Turkish cement industry.

[14]The T-test for the estimated wage difference between the treatment and the control group is statistically significant at the 1% level.

$W_i = W_{i0} + D_i (W_{i1} - W_{i0})$, with only one of W_{i0} and W_{i1} being observed at any given point in time.[15] For all those individuals *treated*, one is interested in estimating the most common parameter in the evaluation literature, $E(W_{i1} - W_{i0}|D_i = 1,\ X_i)$, also referred as the effect of the treatment on the treated.

In social experiments, the evaluation problem is in principle solved, by virtue of random assignment to participation, which guarantees that the potential outcomes are independent of the assignment mechanisms, and then $E(W_{i0}|D_i = 1,\ X_i) = E(W_{i0}|D_i = 0,\ X_i)$. In contrast, in observational studies, assignment is not random, resulting either from individual self-sorting, selection made by a program manager, or both.

In matching, the fundamental assumption, Conditional Independence Assumption (CIA), states that treatment assignment (D_i), conditional on attributes (X_i), is independent of the potential wages (W_{i0}, W_{i1}). In formal notation, this assumption corresponds to

$$(W_{i0}, W_{i1}) \perp D_i \mid X_i, \tag{1}$$

where $\perp$ denotes independence.[16] This means that, given X_i, one can use non-participants' wages to approximate the (counterfactual) wage level of participants had they not participated. Hence, matching consists of finding, for each treated observation, a set of non-treated observations with the same realisation of X_i. In the language of Heckman and Robb (1985), matching assumes that selection occurs only on observables. Therefore, CIA excludes the familiar dependence between outcomes and participation that is central to econometric models of self selection; there are no important variables, apart from X_i (on which the analyst can not condition), that effect both the non-treated outcome (W_{i0}) and the assignment (D_i). If this were the case, then selection would be on unobservables.

A practical implementation problem arises when the vector X_i is highly dimensional and contains continuous variables. To circumvent this difficulty, Rosenbaum and Rubin (1983) show that matching on a scalar function of X_i, such as the propensity score, $P(X_i) = \Pr(D_i = 1|X_i)$, is sufficient to balance the covariates X_i between the treatment and control units. Therefore, if CIA holds conditional on X_i, it will also hold conditional on the propensity score,

$$(W_{i0}, W_{i1}) \perp D_i \mid P(X_i). \tag{2}$$

[15] We are implicitly adopting the stable unit-treatment value assumption (SUTVA) first expressed by Rubin (1980). This assumption requires that an individual's potential outcome is independent of the treatment status of other individuals, ruling out any eventual within-group or spillover (general equilibrium) effect.

[16] "Ignorable treatment assignment", in the terminology of Rubin (1977) and Rosenbaum and Rubin (1983).

In this case, in order to have empirical content, matching also requires

$$0 < P(X_i) = \Pr(D_i = 1|X_i) < 1.^{17} \tag{3}$$

To satisfy this condition, there must be both participants and non-participants for each covariate of the vector X_i. Failure to satisfy this assumption restricts the analysis to the region of support (all possible values of X_i) common to all treated and non-treated units, and the estimated treatment effect has to be redefined as the mean treatment effect for those treated falling within the common region of support.

By construction, matching eliminates two of the three selection bias sources identified by Heckman *et al.* (1998): the bias resulting from having different ranges of X_i for treated and control samples, and the bias resulting from having different distributions of X_i across their common support. The remaining source of bias, differences on unobservables across groups, are ruled out by the matching assumptions.

Under the matching assumptions, the effect of treatment on treated is thus given by,

$$\sum_{i \in D=1} n_i \left(Y_{i1} - \sum_{j \in D=0} N_{ij} Y_{j0} \right), \tag{4}$$

where N_{ij} controls for the weight placed on each comparison observation j for individual i, n_i represents the effective weight for the final treated sample, and Y_{i1} and Y_{j0} stand now for a generic outcome, for the treatment and comparison groups, respectively.

A variety of different matching schemes are possible. Each scheme involves the definition of a closeness criterion, a neighbourhood, and the selection of an appropriate weight function to associate the set of non-treated observations to each participant. For instance, the neighbourhood may range from a singleton set to a multiple set, eventually including all non-treated observations.[18] The choice relies on the trade-off between variance and bias associated with each type of matching performed and the computational intensity allowed. In general, increasing the neighbourhood (or bandwidth) to construct the counterfactual will reduce the variance and increase the bias resulting from using, on average, more, but poorer, matches. It will also rise the computational burden. For selecting the weight function, the most common functions include the unity (equal) weight(s) to the nearest person(s) and

[17] This assumption together with CIA are the "strong ignorability treatment assignment conditions" in the terminology of Rosenbaum and Rubin (1983).

[18] See Heckman *et al.* (1999) and Smith and Todd (2005) for a detailed description of a variety of different matching estimators.

zero to the others, and kernel weights, which downweight distant observations in terms of the propensity score. Silverman (1986) clarifies several alternative kernel functions. A final remark concerns matching with or without replacement, that is, using or not using the same comparison unit repeatedly in forming the comparison group. Similarly, using more than once the same non-treated unit may improve matching quality (reducing the bias), but increase the variance.

In a repeated cross-section or panel context, it is still possible to implement another version of the matching estimator, due to Heckman *et al.* (1997), called nonparametric conditional difference-in-differences. It results from an extension of the conventional difference-in-differences (DiD) estimator by defining outcomes conditional on X_i and using non-(or semi-)parametric methods to construct the differences. The critical identifying assumption, the bias stability condition, using the terminology of Eichler and Lechner (2002), states that, conditional on X_i, the biases are the same, on average, in different time periods before and after the implementation of the program, so that differencing the differences between treated and non-treated units eliminates the bias. Let t and t' denote, respectively, a point in time after and before the program. The effect of treatment on the treated is then identified if $E\left(W_{0t} - W_{0t'} \middle| X_i,\, D = 1\right) = E\left(W_{0t} - W_{0t'} \middle| X_i,\, D = 0\right)$. Thus, the effect of treatment under the bias stability assumption is given by (4) for $Y_{i1} = \left(W_{i1t} - W_{i0t'}\right)$ and $Y_{j0} = \left(W_{j0t} - W_{j0t'}\right)$.

Compared to the original matching estimator, this new version is more robust, since it requires a weaker assumption that allows for an unobserved determinant of participation. Hence, individuals' participation may be based on their potential program outcomes as long as the unobservability (individual and/or time-specific) rests on separable components of the error term. Compared to pure DiD (Meyer, 1995), this estimator has the advantage of being nonparametric, so that successful identification does not depend on specific functional forms for the respective expectations.

4 Data and empirical specifications

The empirical part of this study relies on the Quadros de Pessoal (QP). This is a particularly large and informative data set collected annually by the Portuguese Ministry of Labour and Solidarity since the early eighties. It consists of a matched employer-employee database containing a high number of variables/concepts that meet international standards about each unit, firm or employee, observed. For instance, for each firm the data gives the location, level of employment, economic activity, type of management and total sales. Similarly, for each employee, usual human capital variables, such as gender, level of schooling, tenure, occupation, full-time/part-time status, earnings, length of working time and mechanisms of wage bargaining, among others, are provided. This valuable dataset also includes an identification variable for either the firm or employee observed, which allows us to follow each unit over time.

Before describing the methodology used in this study for creating the data sample and the variables, let us state precisely the treatment effect one is interested in, which will condition the selection of treated and non-treated units. As the *direct* target of the privatisation program is the firm itself and not the employees, one would ideally like to evaluate the privatisation impact on those employees that either remained, joined or left the firm after its privatisation.[19] In this case, for the "joiner or leaver" employee, it would also be required to know the reason for their moving in or out the firm, as the wage accepted by moving individuals varies remarkably according to their employment status. This kind of information is unfortunately unavailable in this dataset, which makes it difficult to interpret the results for these particular two groups. Further, if the employee became unemployed, self-employed or employed by local/central authorities (civil servants), one will not know which, as these organisations are not covered by this survey.

In order to avoid these potential problems, this study focuses only the effect(s) of privatisation on the wages for those employees that remained in the same firm after its privatisation. Therefore, our treated units (employees) correspond to all individuals that work in each public firm subject to privatisation and retain their jobs after the implementation of the reform. To be more precise, let t' and t denote two points in time, representing respectively one period before (pre-treatment) and one after (post-treatment) the privatisation of a given public firm. Thus, the treated group includes all individuals that work both in t' and t for the firm being privatised. The corresponding control (non-treated) group is composed of those workers employed in the remaining public firms (not subject to privatisation) and that, similarly, kept their jobs between t' and t. This choice allows us to match participants with controls not only across certain observable characteristics, but also by pre-treatment public employment status. Thus, we follow the spirit in the evaluation of active labour markets, in which only individuals with common labour market histories (employment) are matched.[20] More importantly, the selection of this particular control group enables us to bypass the self-selection problem inherent in the classical selection model of Heckman (1979) in the context of private and public sectors, and then fully justify the plausibility/adequacy of matching as-

[19] This contrasts with the active labour market policies, in which both the policy and evaluation object targets coincide.

[20] Variables relating labour force status of treated individuals were found to be very significant (even more than earnings) in explaining the participation decision in training programs.

sumptions in the present evaluation. In fact, it has long been recognised that employment in the public or private sector arises from an endogenous decision. Individuals sort themselves in either sector according to their own (mostly unobserved) skills and preferences (in terms of level of risk and complexity of the job, opportunity of internal promotion, quality of the working conditions, etc.), making the public employees a non-random sample from the entire labour force. Because we are using information from the remaining public employees within the same industry for appraising the effects of privatisation, this unobservable component, responsible for the bias, is automatically controlled for. The remaining differences in terms of observable attributes among the public employees will be eliminated by using matching methods.

In addition, note that the purpose of the analysis is to compute the overall impact of privatisation in the banking sector, not firm-by-firm effects. Consequently, the ten firm privatisations need to be condensed into one "single privatisation". The creation of the data sample for estimation is a two step procedure. In the first step, for each privatised firm, we assign two points in time: one pre-treatment t' and one post-treatment t. The respective treated and non-treated individuals are then extracted. The choices of t' and t are driven by economic considerations. Because the firms' process of restructuring occurred mainly after the implementation of the reform, as referred to in Section 2, t' consists of a single calendar year prior to privatisation. In particular, the conventional procedure of the privatisation literature is followed, considering the calendar year of each firm privatisation, the year 0. Therefore $t' = -1$, corresponds to the calendar year prior to each privatisation date. In contrast, for the post-treatment period, we allow privatisation effects to vary over time, following the discussion of Gupta *et al.* (2001). The post-treatment period ranges between one and four years, $t = 1$, 2, 3 and 4, corresponding either to one, two, three or four calendar years after each privatisation date.[21] The second step consists of aggregating, in each t' and t points in time, all treated and non-treated individuals of the respective privatised firms, using a moving window, as shown in Kluve *et al.* (1999). As a result, all individuals, excluding those from the permanent public firms, are considered non-treated and treated at different points in time.

The empirical analysis is based on prime-age individuals not yet subject to retirement. Therefore, the sample is further restricted to individuals aged between 18 and 65 years according to the definition of the vertical collective agreement prevailing in the industry. Apart from these two requirements, only observations without complete demographic information

[21] This post-treatment period choice is also conditioned by the first merger wave in 1998 in the banking industry, which involved recently privatised firms.

in t' and t, used for either the matching algorithm or the outcome equation, were dropped.

As the outcome variable, we use the logarithm of hourly wage, constructed as the logarithm of the sum of monthly base wage, plus the regular and irregular components of the wage, payment indexed to tenure and overtime divided by normal and extra hours worked. Hourly wage is preferable to monthly wage because workers from privatised and public firms experienced different length of working time during the reform. In addition, wages were converted to real terms (1998 prices) using the Consumer Price Index (IPC). Table 2 and 3 display some selected characteristics of the treated and non-treated (potential control) groups segmented by gender, suitable for matching in each time period.

Table 2: Mean attributes for the potential control and treated male groups in time t

	$t=1$		$t=2$		$t=3$		$t=4$	
	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.
Demographic variables*								
Age	43.4	43.5	43.4	43.0	42.6	42.6	39.7	40.9
Tenure	16.6	16.6	17.2	16.3	16.6	15.9	12.9	14.5
Potential experience	27.6	28.3	27.7	27.8	26.6	27.3	23.5	25.6
Education	10.1	9.5	10.3	9.5	10.6	9.3	10.7	9.3
Total working hours per month	143.6	148.3	141.8	147.4	142.4	147.3	145.5	144.7
# months since last promotion	30.5	57.9	28.8	61.0	25.5	61.3	24.0	28.6
Full-time status (%)	96.7	84.9	93.0	81.9	91.0	81.3	94.0	69.1
Occupation (%)								
High skilled	30.1	28.9	31.3	28.2	32.4	27.8	24.0	29.6
Low skilled	69.9	71.1	68.7	71.8	67.6	72.2	76.0	70.4
Region (%)								
North	24.7	21.8	24.2	23.9	.2	23.6	-	7.5
Lisbon and Tagus Valley	70.4	76.3	65.5	76.1	89.4	76.4	96.1	92.5
Madeira and Azores	4.9	1.9	10.3	-	10.4	-	3.9	-
Real hourly wage**								
$t=-1$	1750	1683	1720	1698	1774	1701	1610	1646
	(850)	(869)	(901)	(879)	(976)	(895)	(846)	(1016)
t	1947	1752	1915	1701	2040	1783	1891	2059
	(992)	(828)	(1051)	(733)	(1113)	(713)	(1859)	(990)
Logarithm of real hourly wage**								
$t=-1$	7.40	7.34	7.38	7.35	7.41	7.35	7.32	7.28
	(.345)	(.392)	(.338)	(.397)	(.351)	(.397)	(.333)	(.455)
t	7.50	7.39	7.48	7.37	7.54	7.43	7.49	7.55
	(.361)	(.360)	(.361)	(.343)	(.367)	(.301)	(.300)	(.375)

Source: Own computations based on QP, MSST (1989-1997).
Notes: * Computed at t = -1 for all samples. ** Standard deviation in parentheses.

When the time dimension is controlled for, a striking difference emerge. Considering men (Table 2), the demographic variables indicate that the treated individuals have the same age and experience as the individuals in the control group, are less educated and work more hours. Furthermore, the treated individuals enjoy on average a much longer period without being promoted (5 years) and have a significantly lower fraction of full-time workers than workers in public banks. The only exception is those employees who stay longer within the firm, who tend to be older and more experienced, spend the same time working and are promoted at the same pace as non-treated individuals. Occupational position within the firm reflects differences in the educational level of workers. Therefore, there is a slightly larger proportion of treated individuals in low skilled occupations. Once more, the exception is those employees that remain the longest time within the firm, for whom occupational position reflects seniority instead of educational level. The geographical distribution indicates that the bulk of workers/firms is located in Lisbon. Finally, the difference in the payment level before privatisations mainly reproduces differences in human capital attainment across groups: privatised employees are paid a lower hourly wage than the group of potential controls.

The corresponding figures for women (Table 3) show a very similar picture. Treated women are again less educated, spend more time working, are promoted much less frequently than non-treated women, and have a substantially lower fraction of full time employees. The major difference is that treated women are slightly younger and less experienced than those of the control group. The occupational distribution indicates that treated women have a slightly lower share in the high-skill category. The exception occurs again for those women who stay the longest time period within the firm. Regarding pay levels, the previous pattern applies also for women, with treated women earning less than non-treated women.

The next issue concerns the selection of conditioning variables to be included in X_i in order to estimate the propensity score. In the evaluation of the traditional active labour market policies, the selection of variables in the participation equation is easily conducted by the eligibility requirement rules of each program. In contrast, under the privatisation program, firms, not workers, were selected to be privatised. As mentioned previously, this study assumes that the firm's performance is fully mirrored in the composition and observable quality of the workforce, which is consistent with the well established public-private wage differential literature (see, e.g., Katz and Krueger, 1991, and Disney and Gosling, 1998).[22] Therefore, we include all time constant and time varying attributes of individuals that were

[22] We also tried to include the size of the firm (for the banking sector, this is the only available firm characteristics variable in the data set) in order to control for observable selection of the firms being privatised. Perhaps because all banks are similar in size (with the exception of one bank that is substantially larger than the average), the overall impacts remained unaffected by the inclusion of this variable, affecting (worsening) only the quality of matching.

Table 3: Mean attributes for the potential control and treated female groups in time t

	$t = 1$		$t = 2$		$t = 3$		$t = 4$	
	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.	Cont.	Treat.
Demographic variables*								
Age	39.6	38.9	40.2	42.9	39.8	38.0	37.3	36.0
Tenure	14.1	12.9	15.4	15.4	15.7	12.5	12.4	10.8
Potential experience	24.2	23.9	25.2	23.6	24.3	22.8	21.6	20.8
Education	9.7	9.3	9.6	9.1	10.0	9.7	10.3	9.6
Total working hours per month	139.8	144.5	137.9	144.1	138.6	144.6	139.5	140.8
# months since last promotion	31.1	56.0	30.7	61.1	27.4	61.5	25.3	27.7
Full-time status (%)	90.3	80.0	86.2	77.2	85.1	77.5	86.7	60.2
Occupation (%)								
High skilled	12.7	12.3	12.8	12.0	14.1	12.7	9.7	14.3
Low skilled	87.3	87.7	87.2	88.0	85.9	87.3	90.3	85.7
Region (%)								
North	20.5	23.7	23.1	28.1	3.5	28.47	-	6.5
Lisbon and Tagus Valley	76.0	74.5	70.4	71.9	90.8	71.5	97.7	93.5
Madeira and Azores	3.5	1.9	6.5	-	5.7	-	2.3	-
Real hourly wage**								
$t = -1$	1476 (618)	1326 (629)	1401 (572)	1347 (553)	1470 (574)	1384 (558)	1377 (513)	1280 (591)
t	1578 (822)	1388 (629)	1613 (1134)	1359 (480)	1717 (1274)	1474 (455)	1606 (584)	1629 (700)
Logarithm of real hourly wage**								
$t = -1$	7.23 (.353)	7.12 (.353)	7.18 (.346)	7.14 (.358)	7.23 (.338)	7.17 (.346)	7.18 (.314)	7.08 (.370)
t	7.30 (.345)	7.18 (.320)	7.31 (.361)	7.16 (.309)	7.37 (.363)	7.26 (.260)	7.32 (.348)	7.34 (.322)

Source: Own computations based on QP, MSST (1989-1997).
Notes: * Computed at t = -1 for all samples. ** Standard deviation in parentheses.

not affected by the privatisation reform, such as, schooling, privatisation date, past experience, tenure, occupation and time elapsed since last promotion. For various reasons, the following three variables were not included in our specifications: total working time, location and full-time status. Total working time is our outcome variable, since we compute logarithm of hourly wage, while location reflects the region where the head office of the bank is located instead of the *actual* location of the bank or branch. The inclusion of the variable full-time status violates the assumption (3), as we obtain perfect prediction of being employed in a privatised firm, and destroys the balancing of the variables after matching.[23]

[23] We also tried different specifications for the propensity score, including these and other variables, such as the monthly wage before privatisation, as suggested by the work of Heckman *et al.* (1998). Once again,

Table 4: Results from the participation probit for men and women when t = 1

	Men		Women	
	Coefficient	Std. error	Coefficient	Std. error
Constant	-.198	.317	-.328*	.113
Tenure	.002	.004	.025*	.008
Tenure^2	-.0003*	.0001	-.002*	.0002
Experience	-.057*	.006	-.044*	.006
Experience^2	.001*	.00009	.001*	.0001
Education vs less than 4 years of schooling				
Primary (4)	1.582*	.314	.524*	.082
Preparatory (6)	1.659*	.314	.690*	.091
Lower secondary (9)	1.730*	.314	.919*	.092
Upper secondary (12)	1.210*	.315	.545*	.094
University (16)	1.342*	.316	.519*	.108
# months since last promotion	.005*	.0001	.006*	.0002
Low skilled vs high skilled	-.068*	.017	-.144*	.033
Privatisation date vs 1989				
1991	-.863*	.023	-.169*	.032
1992	-1.221*	.028	-.604*	.040
1993	-.366*	.030	.073**	.044
1994	.054**	.028	.495*	.039
1996	-2.214*	.034	-1.742*	.049
LR chi-squared	13,373	.000$^{b)}$	5,558	.000$^{b)}$
Pseudo R^2	.227		.224	
Fraction correctly predicted (cutoff=.5)	72.67		74.16	
Sample size	44,024		19,957	

Source: Own computations based on QP, MSST (1989-1997).
Notes: *, ** and ***denote significant at the 1, 5 and 10 percent level, respectively. $^{b)}$ P-value for the Likelihood ratio score test for the null hypothesis that all right hand side variables have no effect on privatisation.

Table 4 reports the results of the probit regression for the propensity score correspondent to equation (3) presented in the previous section. We estimate two propensity scores, for men and women respectively, where the binary outcome takes the value 1 if the employee works in a privatised firm when $t = 1$. In total, we estimate eight sets of scores, according to each gender and period of time. Tables 9, 10 and 11 in the Appendix show the probit estimates for $t = 2, 3$, and 4. Table 12 (also in the Appendix) summarises the propensity score obtained for each treatment and control group, across gender and period of time.

The estimation results show, unsurprisingly, that for both genders the conditional participation probability increases with tenure and declines slightly with potential experience (age - schooling - 5). Employees with at least primary school have an increased probability of working in privatised firms. In addition, male or female employees with 6 or 9 years of schooling are clearly more likely to work in a privatised firm. The coefficient estimates for time elapsed since last promotion and occupation are in the expected direction, given the differences observed in Table 2 and 3. Workers with longer periods of time without being promoted and in low skilled occupations are also more likely to be employed by privatised firms. The coefficients on privatisation date reflect the proportion of employees in privatised firms relative to the employees in the control firms. Hence, for both males and females, the magnitude of the effect of privatisation date is positive in 1994 given that in 1989 and 1994, the largest banks in the industry were privatised.

For the actual matching, we also require, beyond the propensity score, that the pool of potential controls, to which a given treated observation may be paired, belong to the same year. We use the Mahalanobis metric for matching in these two variables. By matching within the year we remove explicitly any time-specific unobservables not controlled for by the propensity score, and avoid that each individual is matched with him(her)self. This is the matching analogy to the fixed effects. Also notice that including this variable (privatisation date) both in the propensity score and as additional matching variable amounts to increasing the weight of this variable when forming the matches.

5 Impact estimates

As discussed in Section 3, different matching schemes generate different estimates. This study adopts two estimators which are extreme in terms of neighbourhood size.[24] We adopt the

the magnitude of the impacts remains unaffected by the inclusion of these variables in the specification, affecting (worsening) only the quality of matching.

[24] The matching estimates were obtained using command psmatch2 in Stata, written by Leuven and Sianesi (2003).

nearest-neighbour matching estimator and the Gaussian kernel estimator, without imposing any restriction in the region of common support.[25] In terms of notation from equation (4), we define $N_{ij} = 1$ for the nearest-neighbour matching estimator, since each treated individual is matched with the closest non-treated individual. For the Gaussian kernel estimator, the outcome for each treated unit i is matched with a kernel-weighted average of outcomes for all non-treated individuals, where the weight given to the non-treated unit j is in proportion to the closeness, in terms of propensity score, between i and j. Formally, the outcome Y_{j0} is weighted by $N_{ij} = \frac{K\left(\frac{p_i - p_j}{h}\right)}{\sum_{j \in D=0} K\left(\frac{p_i - p_j}{h}\right)}$, where $K(\cdot)$ is based on the Gaussian distribution, p is the propensity score and h is size of the neighborhood, i.e., the bandwidth. We chose $h = .06$.[26] The overall effect of privatisation in both cases is given by the arithmetic mean of all individual effects and n_i is thus given by the inverse of the sample size of the treated group. Overall, matching with the nearest-neighbour or with the kernel estimators on the estimated propensity score reduces substantially the variability in observable attributes, whether measured by the median absolute bias or the pseudo R^2 (see Table 13 in the Appendix).

Table 5 reports the impact of privatisation on the logarithm of hourly wage for men, over four different time periods, using four different matching strategies. In the first two rows, we present results from the nearest-neighbour and kernel matching estimators, implemented in the context of cross-section samples. The privatisation effects are estimated using equation (4) for $Y_{i1} = W_{i1t}$ and $Y_{j0} = W_{i0t}$. In the last two rows, these two same matching estimators are reproduced under weaker assumptions, using longitudinal data. We estimate equation (4) for $Y_{i1} = (W_{i1t} - W_{i0t'})$ and $Y_{j0} = (W_{j0t} - W_{j0t'})$. We also present the estimates obtained with a parametric difference-in-differences estimator, using the same control group as in Monteiro (2004). The pre-program period for each privatised firm is given by $t = -1$, while the post-program period is given by t, ranging between one and four years. For example, the figure -.064 (first row, first column) indicates that during the first year post-reform, the wage paid to retained men in privatised firms grew 6.2 percent $(e^{-.064} - 1)$ less than the wage paid to their respective counterparts in public firms.

The overall picture depicted in Table 5 and 6, and Figure 1 and 2, confirms the dynamic of the privatisation effects formerly identified in Monteiro (2004). In fact, in contrast with prior evidence, both tables and figures reveal that the privatisation effects vary in sign and magnitude according to the time of evaluation. Moreover, both figures seem to suggest a *positive* relationship between time of restructuring and relative growth rate of wages.

[25] Table 12 in the Appendix shows that lack of common support is not an issue in the present evaluation.
[26] The results remain qualitatively unchanged if we adopt different neighborhood sizes for each participant.

Figure 1: The impact of privatisation on the hourly wage of men

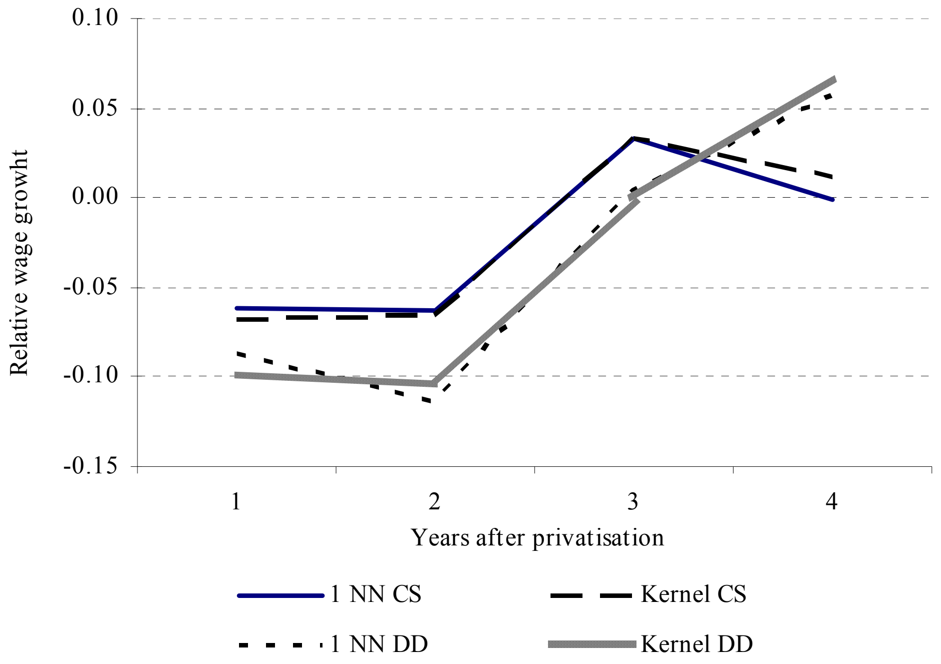

Figure 2: The impact of privatisation on the hourly wage of women

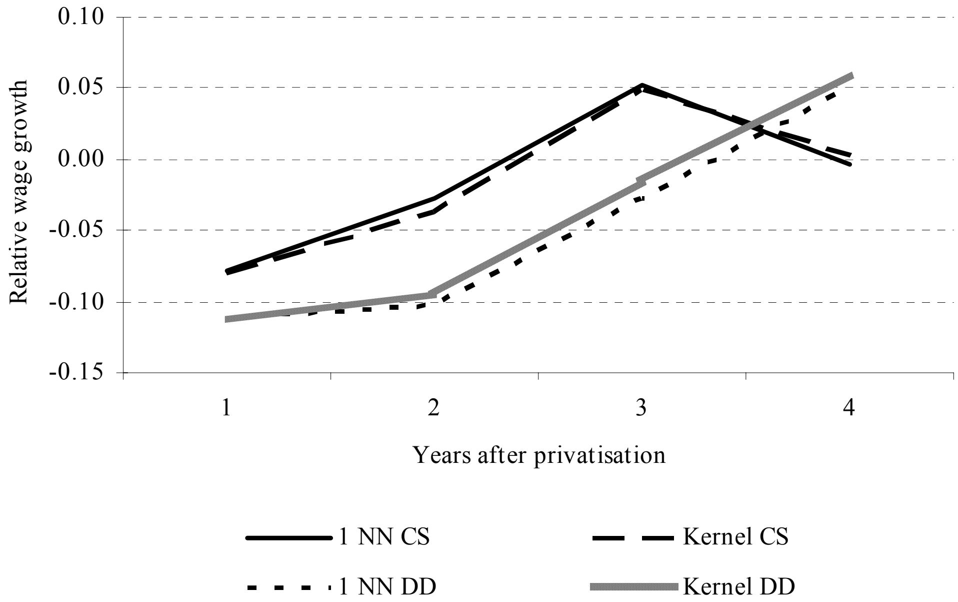

Table 5: Matching estimates of the impact of privatisation on log hourly wage of men

Matching version	Time effect + 1 year	+ 2 years	+ 3 years	+ 4 years
Cross section				
1 NN	−.064** (.030)	−.065*** (.040)	.033 (.045)	−.0004 (.009)
Kernel	−.070* (.025)	−.068*** (.036)	.032 (.028)	.011*** (.006)
Longitudinal				
1 NN	−.091* (.026)	−.120* (.034)	.004 (.036)	.056* (.007)
Kernel	−.105* (.021)	−.111* (.030)	−.001 (.023)	.064* (.005)
Parametric Difference-in-Differences	−.092* (.003)	−.087* (.004)	−.056* (.004)	.043* (.007)
Treated sample size	17, 210	13, 912	12, 726	6, 801

Source: Own computations based on QP, MSST (1989-1997).
Notes: Standard errors in parentheses.*, ** and *** denote statistically significant from zero at the 1, 5 and 10 percent levels.

During the first two years after privatisation, both men and women suffer lower wage growth rates, but this development tends to be reversed in the subsequent periods. Consequently, this result supports the general objective of restructuring (cost reduction) implicit in the implementation of the policy, and indirectly confirmed by Pinho (1999). As previously referred, the Portuguese banking industry experienced a significant efficiency improvement during the period 1988-97, particularly among the privatised institutions. Our short-term wage effects are therefore consistent with the findings of McGuckin and Nguyen (2001) regarding the effects of ownership changes in the US manufacturing sector: around 76% of employees enjoyed lower wage growth rates after the ownership change. On the other hand, deregulation of the product market – a related policy implemented in order to increase the degree of product market competition – leads in general to declines in the wage growth rate. For example, Black and Strahan (2001) find that, in the US banking industry, male wages fell by 12.5 per cent.

Three years post-reform, the matching impacts of privatisation are mixed and insignificant, whereas, in the fourth year, retained employees are in advantage if longitudinal matching estimates or previous results are considered. For this period of analysis, the results are then consistent with those found by Parker and Martin (1996), despite the fact that their analysis includes the entire workforce, regardless of gender. These authors find that four or five years after privatisation, wages, on average, had increased (up to 8.4 percent) in 7 out of 11 privatised firms in the UK, when compared to the whole economy.

These results seem to suggest a change in the pay policy of privatised firms. After firms

Table 6: Matching estimates of the impact of privatisation on log hourly wage of women

Matching version	Time effect	+ 1 year	+ 2 years	+ 3 years	+ 4 years
Cross section					
1 NN		−.081* (.029)	−.028 (.036)	.051 (.042)	−.003 (.015)
Kernel		−.083* (.024)	−.037 (.032)	.048 (.034)	.003 (.010)
Longitudinal					
1 NN		−.118* (.023)	−.108* (.033)	−.027 (.036)	.050* (.012)
Kernel		−.121* (.020)	−.101* (.028)	−.016 (.029)	.057* (.009)
Parametric Difference-in-Differences		−.062* (.004)	−.115* (.008)	−.056* (.006)	.043* (.007)
Treated sample size		6, 235	4, 967	4,486	2,151

Source: Own computations based on QP, MSST (1989-1997).
Notes: Standard errors in parentheses.*, ** and *** denote statistically significant from zero at the 1, 5 and 10 percent levels.

have completed the main adjustments, elimination of redundant workers – consistent with figures on elapsed time since last promotion from Table 2 and 3 – and reduction of wage growth, the remaining labour force is better rewarded. The rise in wage growth rates following the adjustment period corresponds well with predictions from theoretical privatisation models that include effort or efficiency wages (Haskel and Sanchis, 1995, and Goerke, 1998). Workers might have exerted a higher level of effort, also acknowledged in the bargaining contract, and thus increased productivity, as a response to increased fears of dismissal, given the uncertainty caused by the reform.

A point worth noting concerns the performance of the four matching estimators implemented. Both longitudinal matching estimators tend to overestimate the impact found by the corresponding cross-sectional estimator, and the difference between the estimators increases with time after the reform. Maybe the relatively small number of variables used in this study, compared with the studies of evaluation of employment and training programs, may explain the different performance between cross-sectional and longitudinal estimators. Alternatively, this difference may indicate the implausibility of the cross-sectional matching assumptions for analysing long term effects. Recall that in a pure random experiment different methodological strategies would yield similar results within each time period. Thus, there might be some unobservable or observable variables, not accounted for, that are contaminating the results.

We turn now to the question of identifying sources of heterogeneity, other than gender and timing, for which privatisation effects are most prominent. Therefore, Table 7 and

Table 8 report results obtained from the nearest neighbour difference-in-differences matching estimator for men and women, respectively, for different groups stratified according to age, tenure, education, occupation and full-time status.

Table 7: The impacts of privatisation on the log hourly wage of men

DiD matching	Time effect	+ 1 year	+ 2 years	+ 3 years	+ 4 years
Age					
[18 – 30[		.030	.009	.027	.089**
		(.029)	(.046)	(.057)	(.037)
[30 – 50[		−.084*	−.130*	.022	.070*
		(.022)	(.034)	(.037)	(.008)
[50 – 65]		−.124**	−.134***	−.093	−.056**
		(.062)	(.070)	(.073)	(.027)
Tenure					
[0 – 10[		−.026**	−.131*	.019	.089*
		(.011)	(.025)	(.038)	(.013)
[10 – 20[		−.067***	−.109**	.065***	.074*
		(.035)	(.045)	(.049)	(.011)
[20 – [		−.138*	−.114**	.060	−.013
		(.046)	(.052)	(.066)	(.018)
Education					
[0 – 6[		−.163*	−.138*	−.032	−.0004
		(.027)	(.029)	(.034)	(.030)
[6 – 16[		−.083*	−.121*	.020	.069*
		(.027)	(.035)	(.041)	(.008)
[16 – [		−.164*	−.183*	−.118***	−.068**
		(.037)	(.053)	(.068)	(.039)
Occupation					
High skilled		−.066*	−.095*	−.053***	.006
		(.020)	(.028)	(.041)	(.015)
Low skilled		−.106*	−.121*	.036	.090*
		(.032)	(.041)	(.048)	(.009)
Treated sample size		17,210	13,912	12,726	6,801

Source: Own computations based on QP, MSST (1989-1997).
Notes: Standard errors in parentheses. *,** and *** denote statistically significant from zero at the 1, 5 and 10 percent levels.

It turns out that assuming a single privatisation effect per time period masks, to a considerable extent, the variation of privatisation effects. Nevertheless, although significant differences across and within skill groups arise from these two tables, a fairly similar positive trend can be detected for most skill groups, regardless of gender. Figure 3 helps to uncover these trends.

Starting with age, the results indicate that privatisation penalised relatively more the oldest employees of both genders. In fact, employees aged 50+ years experienced significant wage losses during the first two years, clearly more pronounced than the average of the respective gender group within each time period. This may not be particularly surprising, as the workers in this age-group were relatively close to retirement. On the other hand, whereas compulsory wage promotions are defined for the initial years of the career by the wage agreement contract, these are optional for the latter years of the career and for the

Figure 3: The impact of privatisation on the hourly wage by gender, across age, tenure, education and occupational groups.

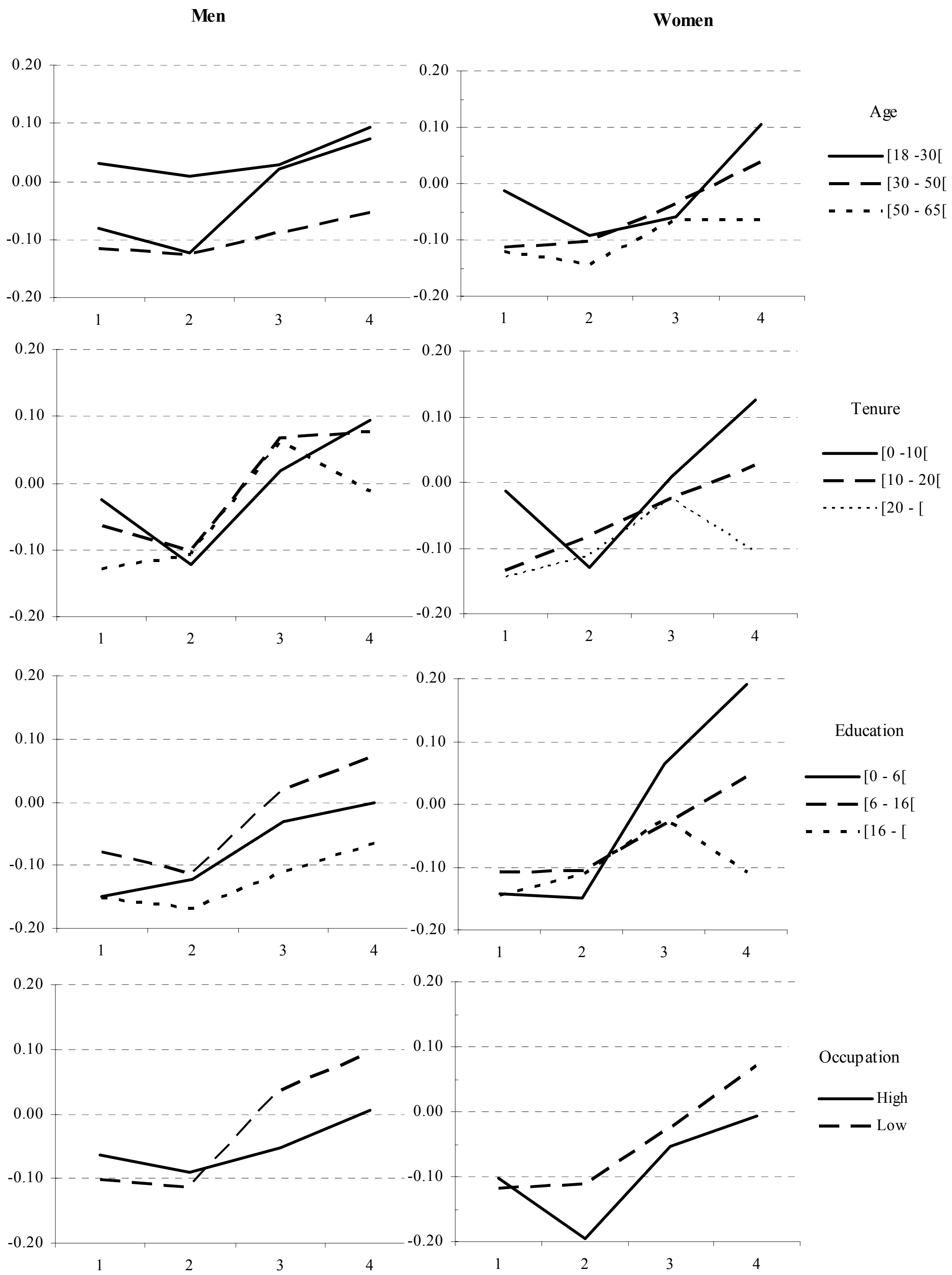

Table 8: The impacts of privatisation on the log hourly wage of women

DiD matching	Time effect	+ 1 year	+ 2 years	+ 3 years	+ 4 years
Age					
[18 − 30[		−.013	−.096*	−.061	.100*
		(.026)	(.046)	(.068)	(.035)
[30 − 50[		−.121*	−.108*	−.037	.038*
		(.029)	(.040)	(.041)	(.013)
[50 − 65]		−.127*	−.155*	−.065	−.067
		(.036)	(.048)	(.071)	(.085)
Tenure					
[0 − 10[		−.014	−.140*	.009	.118*
		(.013)	(.029)	(.031)	(.020)
[10 − 20[		−.144*	−.084***	−.025	.025***
		(.037)	(.046)	(.055)	(.019)
[20 − [		−.156*	−.119***	−.024	−.114*
		(.049)	(.071)	(.080)	(.037)
Education					
[0 − 6[		−.155*	−.163*	.062***	.175*
		(.022)	(.027)	(.035)	(.053)
[6 − 16[		−.115*	−.133*	−.033	.042*
		(.027)	(.035)	(.041)	(.012)
[16 − [		−.142**	−.117	−.079	−.004
		(.080)	(.118)	(.086)	(.064)
Occupation					
High skilled		−.108*	−.217*	−.054	−.006
		(.029)	(.045)	(.049)	(.035)
Low skilled		−.125*	−.117*	−.025	.068*
		(.026)	(.037)	(.041)	(.013)
Treated sample size		6,235	4,967	4,486	2,151

Source: Own computations based on QP, MSST (1989-1997).
Notes: Standard errors in parentheses. *,** and *** denote statistically significant from zero at the 1, 5 and 10 percent levels.

highest paid occupations within the firm. Therefore, results suggest that firms cut wages for the oldest individuals, while rewarding the youngest employees.

Evidence on tenure subgroups also reflects this restriction of the firms' freedom to set wages for certain experienced groups. Individuals who remained the longest time within the firm suffered the highest wage losses over a longer time period, while the younger individuals enjoyed the highest wage gains.

Looking at educational breakdowns, a surprising result is displayed. In contrast with our expectation, the best educated male and female employees are the most negatively affected sub-groups in the workforce, suffering sharp and lasting reductions (which are never reversed) in their relative wages, in particular after two years of the implementation of the reform. A possible explanation for this finding might relate to the unknown size of the non-cash component of compensation paid to this group. Given tax allowances, firms might have preferred to reduce their relative wage and allow employees to still enjoy generous non-wage compensation, such as free car. On the other hand, since banks incurred substantial expenses in continuous job-training programs to update an old workforce, formal education might have become relatively less relevant in terms of pay policy.

Finally, in what concerns occupational sub-categories, both tables indicate that, during the first two years, privatisation had a negative impact on relative wages for both low- and high-skilled workers. Yet, in contrast with the low-skilled category, the high-skilled workers never enjoyed a relative wage increase. Hence, despite the broad concept of managers used here, this result seems to contradict the positive prediction (from a variety of theories) of the impacts of privatisation on CEO pay levels.[27] The reason for this finding is likely to be related to the downsizing strategy of privatised firms, possibly implying a lower level of supervision and responsibilities for employees in this occupational category.

6 Concluding Remarks

The causal effect of privatisation on wages remains an important and controversial topic among policy-makers and economists alike. The frequent opposition from public opinion and trade unions towards privatisation programs makes this particular topic a challenging issue for policy makers. The resistance usually arises from the fear of adverse labour strategies, including either displacement or wage reductions. For economists, on the other hand, the topic creates additional interest, for at least two different reasons. First, different theoretical approaches produce ambiguous predictions regarding the wage effects of privatisation. Second, there is the habitual missing data problem inherent in the evaluation of causal effects in observational studies. In contrast with active labour market policies, though, privatisation has not been the target of a lively discussion from an evaluation standpoint, and therefore deserves further scrutiny.

The purpose of this paper has been to investigate the effects of privatisation on wages in the Portuguese banking industry. In particular, we were interested in testing if earlier findings on privatisation wage effects (Monteiro, 2004) are robust to the selection of methodology. Following earlier analysis, we focus on the effects of privatisation on employees who remained within the firm after the reform, by comparing wages of those employees with employees in public firms. We have done this by implementing two variants of matching estimators in two different contexts: cross-sectional and longitudinal samples.

In general, the results point to an overall confirmation of previous findings. Indeed, our results, obtained from *Quadros de Pessoal* for the period between 1989 and 1997, generally show a negative (positive) short-run (long-run) effect of privatisation on relative wage growth for both men and women retained in the privatised firms. When the wage effects are broken

[27] See Rosen (1992) or Wolfram (1998) for a theoretical and empirical survey on executive pay levels.

down to account for the heterogeneity of the effects, a persistent positive pattern prevails, irrespective of gender. The evidence provided here also shows that the restructuring process hit more intensively the most educated employees. This surprising result, which contrasts with the conventional wisdom from the public/private wage literature, may also imply that, rather than education, seniority and experience still count for much in this particular labour market.

Our results have at least two important policy implications. First, privatisation seems to be a gender neutral policy, given the similarity of the effects by gender, both in terms of trend and intensity. Thus, our results appear to contradict Gary Becker's prediction about the relationship between market structure and discrimination. Nevertheless, more research is clearly needed to assess if privatisation affects men and women differently. Second, the evidence presented so far also shows that the fear of wage cuts following privatisation – as is often argued by labour unions – seems to be unfounded. Indeed, wage losses – if they occur – are only temporary, as the long-term dynamics seem to confirm the law of one price in the labour market.

References

[1] Al-Obaidan, Abdullah M., "Efficiency Effect of Privatization in the Developing Countries", *Applied Economics* 34:1(2002), 111-117.

[2] Aperta, Anabela, Isaura Moreira, and Maria Murteira, *Análise das Diferenciações entre Remunerações Convencionais e Efectivas*, (Lisboa: Ministério do Emprego e da Segurança Social, Colecção Estudos, série B-Rendimento, 1994).

[3] Black, Sandra and Philip Strahan, "The Division of Spoils: Rent-Sharing and Discrimination in a Regulated Industry", *American Economic Review* 91:4 (2001), 814-831.

[4] Blundell, Richard and Mónica Dias, "Evaluation Methods for Non-Experimental Data", *Fiscal Studies* 21:4 (2000), 427-448.

[5] Brainerd, Elizabeth, "Five Years after: The Impact of Mass Privatization on Wages in Russia, 1993-1998", *Journal of Comparative Economics* 30 (2002), 160-190.

[6] Cam, Surhan, "Job security, unionisation, wages and privatisation: a case study in the Turkish cement industry", *The Sociological Review* 47:4 (1999), 695-714.

[7] Cerdeira, Maria, *A Evolução da Sindicalização Portuguesa de 1974 a 1995*, (Lisboa: Ministério para a Qualificação e o Emprego, Colecção Estudos, série C-Trabalho, 1997).

[8] De Fraja, Giovanni, "Unions and Wages in Public and Private Firms: A Game-Theoretic Analysis", *Oxford Economic Papers* 45 (1993), 457-469.

[9] Disney, Richard and Amanda Gosling, "Does it pay to work in the public sector?",

Fiscal Studies 19:4 (1998), 347-374.

[10] Eichler, Martin and Michael Lechner, "An Evaluation of Public Employment Programmes in the East German State of Sachsen-Anhalt", *Labour Economics* 9:2 (2002), 143-186.

[11] Goerke, Laszlo, "Privatization and Efficiency Wages", *Journal of Economics* 67:3 (1998), 243-264.

[12] Gupta, Sanjeev, Christian Schiller, Henry Ma and Erwin Tiongson, "Privatization, Labor and Social Safety Nets", *Journal of Economic Surveys* 15:5 (2001), 647-669.

[13] Haskel, Jonathan and Stefan Szymanski, "A Bargaining Theory of Privatisation", *Annals of Public and Cooperative Economics* 63 (1992), 207-227.

[14] Haskel, Jonathan and Stefan Szymanski, "Privatization, Liberalization, Wages and Employment: Theory and Evidence for the UK", *Economica* 3 (1993), 161-182.

[15] Haskel, Jonathan and Amparo Sanchis, "Privatisation and X-Inefficiency: a Bargaining Approach", *Journal of Industrial Economics* 43:3 (1995), 301-321.

[16] Heckman, James, "Sample Selection Bias as a Specification Error", *Econometrica* 47:1 (1979), 153-161.

[17] Heckman, James J., Hidehiko Ichimura, Jeffrey Smith and Petra Todd, "Characterising Selection Bias Using Experimental Data", *Econometrica* 66:5 (1998), 1017-1098.

[18] Heckman, James, Hidehiko Ichimura, and Petra Todd, "Matching As an Econometric Evaluation Estimator: Evidence from Evaluating a job Training Programme", *Review of Economic Studies* 64:4 (1997), 605-654.

[19] Heckman, James, Hidehiko Ichimura, and Petra Todd, "Matching As an Econometric Evaluation Estimator", *Review of Economic Studies* 65:2 (1998a), 261-294.

[20] Heckman, James, Robert Lalonde and Jeffrey Smith, "The Economics and Econometrics of Active Labour Market Programmes", in O. Ashenfelter and D. Card (Eds.), *The Handbook of Labour Economics*, vol. III (Amsterdam: North Holland, 1999).

[21] Heckman, James, and Richard Robb, "Alternative Methods for Evaluating the Impact of Interventions," in J. Heckman and B. Singer (Eds.), *Longitudinal Analysis of Labour Market Data* (Cambridge University Press, New York: 1985).

[22] Ho, Samuel P., Xiao-Yuan Dong, Paul Bowles and Fiona MacPhail, "Privatization and enterprise Wage Structures during Transition, Evidence from Rural Industry in China", *Economics of Transition* 10:3 (2002), 659-688.

[23] Katz, Lawrence and Alan Krueger, "Changes in the Structure of Wages in the Public and Private Sectors", in Ehrenberg, R. (Ed.), *Research in Labour Economics*. (JAI Press,CT, Greenwhich: 1991, 137-172).

[24] Kluve, Jochen, Hartmut Lehmann and Christoph Schimdt, "Active Labor Policies in Poland: Human Capital Enhancement, Stigmatization, or Benefit Churning?", *Journal of Comparative Economics* 27 (1999), 61-89.

[25] Kraft, Evan, Richard Hofler and James Payne, "Privatization, Foreign Bank Entry and Bank Efficiency in Croatia: a Fourier-flexible Function Stochastic Cost Frontier Analysis", Applied Economics, 38:17 (2006), 2075-2088.

[26] La Porta, Rafael and Florencio Lopez-de-Silanes, "Benefits of Privatization-Evidence from Mexico", *Quarterly Journal of Economics* 114:4 (1999), 1193-1242.

[27] Leuven, E. and Barbara Sianesi, "PSMATCH2: Stata Module to Perform full Mahalanobis and Propensity Score Matching, Common Support Graphing, and Covariate Imbalance Testing", (2003), http://ideas.repec.org/c/boc/bocode/s432001.html, this version 3.0.0.

[28] McGuckin, Robert and Sang Nguyen, "The Impact of Ownership Changes: A View from Labor Markets", *International Journal of Industrial Organization* 19:5 (2001), 739-62.

[29] Megginson, William, Robert Nash, and Matthias van Randenborgh, "The Financial and Operating Performance of Newly Privatized Firms: An International Empirical Analysis", *Journal of Finance* 49:2 (1994), 403-452.

[30] Megginson, William and Jeffrey Netter, "From State to Market: A Survey of Empirical Studies on Privatization", *Journal of Economic Literature* 39:2 (2001), 321-389.

[31] Meyer, Bruce, "Natural and Quasi-Experiments in Economics", *Journal of Business and Economic Statistics* 12:2 (1995), 151-161.

[32] Ministério da Segurança Social e do Trabalho. Departamento de Estatística. Quadros de Pessoal, data in magnetic media.(QP, MSST, Universidade do Minho, Braga, Portugal, 1989 to 1997).

[33] Ministério da Segurança Social e do Trabalho, Departamento de Estatística do Trabalho, Emprego e Formação Profissional, *Greves Anual. Informação Estatística (Síntese)*, (MSST, DETEFP, Lisboa, Portugal, 1980 to 2000).

[34] Ministério da Segurança Social e do Trabalho, Departamento de Estudos, Prospectiva e Planeamento, *Colecção Relatórios e Análises, Série Regulamentação do Trabalho. Aumento Médio Ponderado Intertabelas e Aumentos Intertabelas Deflacionados.* (MSST, DEPP, Lisboa, Portugal, 1980-2000).

[35] Monteiro, Natália P., "The Impact of Privatisation on Wages: Evidence from the Portuguese Banking Industry", NIPE Working Paper No. 11, (2004).

[36] Naumann, Reinhard, *Privatizações e Reestruturações. O Desafio para o Movimento Sindical em Portugal*, (Lisboa: Fundação Friedrich Ebert, 1995).

[37] Organisation for Economic Cooperation and Development, *OECD Economic Surveys, Portugal, 1988-89*, (Paris: 1989).

[38] Organisation for Economic Cooperation and Development , *OECD Economic Surveys, Portugal, 1990-91*, (Paris: 1991).

[39] Organisation for Economic Cooperation and Development , *OECD Economic Surveys, Portugal, 1998-99* (Paris: 1999).

[40] Parker, David and Stephan Martin, "The Impact of UK Privatization on Employment, Profits and the Distribution of Business Income", *Public Money & Management* 16:1 (1996), 31-37.

[41] Peoples, James and Wayne Talley, "Black-White Earnings Differentials: Privatization versus Deregulation", *American Economic Review* 91:2 (2001), 164-173.

[42] Pinho, Paulo, "Reprivatizações e Eficiência no Sistema Bancário Português", Documento de Trabalho n^{o} 13, (Lisboa: Direcção-Geral de Estudos e Previsão, Ministério das Finanças, 1999).

[43] Rosen, Sherwin, "Contracts and the Market for Executives", in Lars Werin and Hans Wijkander (Eds), *Contract Economics* (Oxford: Basil Blackwell 1992).

[44] Rosenbaum, Paul and Donald Rubin, "The Central Role of the Propensity Score in Observational Studies for Causal Effects", *Biometrika* 70:1 (1983), 41-55.

[45] Rubin, Donald, "Assignment to a Treatment Group on the Basis of a Covariate", *Journal of Educational Statistics* 2:1 (1977), 1-26.

[46] Rubin, Donald, "Randomization Analysis of Experimental Data in the Fisher Randomization Test by D.Basu", *Journal of the American Statistical Association* 75 (1980), 591-593.

[47] Silverman, Bernard, *Density Estimation for Statistics and Data Analysis,* (Chapman and Hall, 1986).

[48] Smith, Jeffrey and Petra Todd, "Does Matching Overcome LaLaonde's Critique of Nonexperimental Estimators?", *Journal of Econometrics* 125 (2005), 305-353.

[49] Sousa, Fernando and Ricardo Cruz, *O Processo de Privatizações em Portugal* (Porto: Associação Industrial Portuense, 1995).

[50] Wolfram, Catherine, "Increases in Executive Pay Following Privatization", *Journal of Economics & Management Strategy* 7:3 (1998), pp. 327-361.

Appendix

Table 9: Results from the participation probit for men and women when t = 2

	Men		Women	
	Coefficient	Std. error	Coefficient	Std. error
Constant	.288	.454	.087	.144
Tenure	.011	.006	.027*	.010
Tenure^2	-.001*	.0001	-.002*	.0003
Experience	-.073*	.008	-.053*	.008
Experience^2	.001*	.0001	.001*	.0001
Education vs less than 4 years of schooling				
Primary (4)	2.297*	.449	.646*	.103
Preparatory (6)	2.495*	.449	.987*	.117
Lower secondary (9)	2.642*	.449	1.383*	.119
Upper secondary (12)	1.867*	.449	.903*	.121
University (16)	2.130*	.451	1.002*	.143
# months since last promotion	.007*	.0002	.008*	.0003
Low skilled vs high skilled	-.109*	.022	-.163*	.043
Privatisation date vs 1989				
1991	-1.692*	.043	-.759*	.043
1992	-1.782*	.047	-.814*	.052
1993	-1.560*	.046	-.747*	.051
1994	-.991*	.045	.122*	.049
LR chi-squared	6,439	.000$^{b)}$	2,494	.000$^{b)}$
Pseudo R^2	.209		.178	
Fraction correctly predicted (cutoff=.5)	70.50		68.32	
Sample size	22,994		10,110	

Source: Own computations based on QP, MSST (1989-1997).
Notes: *, ** and ***denote significant at the 1, 5 and 10 percent level, respectively. $^{b)}$ P-value for the Likelihood ratio score test for the null hypothesis that all right hand side variables have no effect on privatisation.

Table 10: Results from the participation probit for men and women when t = 3

	Men		Women	
	Coefficient	Std. error	Coefficient	Std. error
Constant	.177	.519	.098	.177
Tenure	.044*	.008	.050*	.011
Tenure^2	-.002*	.0002	-.004*	.0003
Experience	-.083*	.001	-.045*	.011
Experience^2	.002*	.0001	.001*	.0001
Education vs less than 4 years of schooling				
Primary (4)	2.476*	.513	.355	.137
Preparatory (6)	2.926*	.513	.933*	.150
Lower secondary (9)	3.093*	.513	1.502*	.153
Upper secondary (12)	2.216*	.513	.922*	.155
University (16)	2.578*	.516	1.119*	.176
# months since last promotion	.010*	.0003	.010*	.0004
Low skilled vs high skilled	-.131*	.027	-.179*	.047
Privatisation date vs 1989				
1991	-1.629*	.063	-.586*	.056
1992	-2.282*	.065	-1.148*	.058
1993	-1.884*	.065	-.849*	.058
1994	-1.582*	.064	-.411*	.056
LR chi-squared	5,777	.000$^{b)}$	2,642	.000$^{b)}$
Pseudo R^2	.252		.234	
Fraction correctly predicted (cutoff=.5)	76.71		73.81	
Sample size	18,492		8,189	

Source: Own computations based on QP, MSST (1989-1997).
Notes: *, ** and ***denote significant at the 1, 5 and 10 percent level, respectively. $^{b)}$ P-value for the Likelihood ratio score test for the null hypothesis that all right hand side variables have no effect on privatisation.

Table 11: Results from the participation probit for men and women when t = 4

	Men		Women	
	Coefficient	Std. error	Coefficient	Std. error
Constant	-5.514	-	.986*	.217
Tenure	.074*	.007	.082*	.012
Tenure^2	-.002*	.0002	-.004*	.0004
Experience	-.107*	.009	-.102*	.012
Experience^2	.002*	.0001	.002*	.0002
Education vs less than 4 years of schooling				
Primary (4)	6.125*	.108	-.352***	.186
Preparatory (6)	6.533*	.102	-.247	.193
Lower secondary (9)	6.453*	.096	.032	.197
Upper secondary (12)	5.796*	.089	-.508**	.198
University (16)	5.743*	.094	-.899*	.215
# months since last promotion	.006*	.0005	.003*	.0007
Low skilled vs high skilled	-.316*	.028	-.553*	.054
Privatisation date vs 1989				
1991	.729*	.027	.630*	.043
1992	.273*	.033	.302*	.048
1993	-.183*	.039	-.477*	.068
LR chi-squared	2,426	.000$^{b)}$	970	.000$^{b)}$
Pseudo R^2	.121		.113	
Fraction correctly predicted (cutoff=.5)	68.10		72.63	
Sample size	14,490		6,965	

Source: Own computations based on QP, MSST (1989-1997).
Notes: *, ** and ***denote significant at the 1, 5 and 10 percent level, respectively. $^{b)}$ P-value for the Likelihood ratio score test for the null hypothesis that all right hand side variables have no effect on privatisation.

Table 12: Propensity scores by time and gender

		Mean	Std. Dev.	Min	Max
$t = 1$	Men				
	Privatised	.5537567	.2340450	.0131557	.9887109
	Public	.2869447	.2071896	.0018966	.9815215
	Women				
	Privatised	.4816485	.220088	.0129936	.9554378
	Public	.2371310	.1924759	.0006537	.9402833
$t = 2$	Men				
	Privatised	.6999680	.2265299	.1025044	.9988026
	Public	.4588628	.1831909	.0026444	.9919025
	Women				
	Privatised	.6045986	.2280892	.0085209	.9891979
	Public	.3841890	.1891185	.0166561	.9756401
$t = 3$	Men				
	Privatised	.7728200	.2070783	.0657026	.9999654
	Public	.4998144	.2015494	.0018765	.9998800
	Women				
	Privatised	.6755556	.227302	.0376332	.9970115
	Public	.3934719	.2194256	.0055463	.9912833
$t = 4$	Men				
	Privatised	.5536247	.1905893	.0830682	.9645761
	Public	.3953927	.1732543	6.07e-12	.9202089
	Women				
	Privatised	.3995526	.1598363	.0147413	.8847555
	Public	.2674113	.1518061	.0007223	.8765878

Table 13: Indicators of covariate balancing, before and after matching, by time and gender

			Probit ps.- R^2 before	Probit ps.- R^2 after	Pr $>\chi^2$ after	Median absol. bias before	Median absol. bias after
t=1	Men	1NN	.227	.010	.000	12.032	2.923
		Kernel	.227	.014	.000	12.032	2.728
	Women	1NN	.224	.007	.000	14.083	1.434
		Kernel	.224	.004	.000	14.083	1.694
t=2	Men	1NN	.209	.025	.000	13.107	.3.673
		Kernel	.209	.027	.000	13.107	6.220
	Women	1NN	.178	.009	.000	24.137	6.244
		Kernel	.178	.007	.000	24.137	2.834
t=3	Men	1NN	.252	.042	.000	13.152	8.157
		Kernel	.252	.047	.000	13.152	4.385
	Women	1NN	.234	.018	.000	32.547	6.717
		Kernel	.234	.011	.000	32.547	4.454
t=4	Men	1NN	.121	.003	.000	21.094	2.788
		Kernel	.121	.002	.000	21.094	2.327
	Women	1NN	.113	.003	.184	12.241	1.376
		Kernel	.113	.004	.077	12.241	2.333